2.0 THE INITIAL OR "PRIOR" DEGREE OF CONFIDENCE OF A POSSIBLE EMF HAZARD

2.1 TO WHAT HYPOTHESES DO THE DHS SCIENTISTS' PRIOR PROBABILITIES REFER?

As mentioned above, developing a prior probability is unavoidably subjective and an issue of hot debate among statisticians. Although the reviewers' priors were not used as a mechanical multiplier to derive a posterior, presenting the priors does reveal explicitly the assumptions of the reviewers and allows the reader to see how much the EMF-specific evidence has moved the three reviewers from their *a priori* degree of confidence. In particular, the reviewers wanted to address explicitly whether the biophysical arguments make their prior vanishingly small and how their prior for EMFs compares to that for other environmental agents.

The posterior degrees of confidence, on the other hand, were elicited directly, after a structured consideration of the EMF-specific evidence. The three core reviewers did their best to separate out what could have been known or discussed in 1979 before the publication of Wertheimer and Leeper's first paper on alleged power line effects and use only that prior knowledge to form their prior degrees of confidence. For example, the extensive dialogue on the biophysical credibility of a noticeable physical induction of molecular changes from residential EMFs emerged after 1979. However, it was based on knowledge available before 1979 and could have taken place then, so it was considered relevant to the prior. EMF-specific epidemiological, mechanistic, and animal pathology results were excluded from discussion.

The three reviewers also discussed environmental agents in general and tried to anchor and compare their EMF priors to their "general" priors. In this way they tried to avoid having EMF-specific information influence their priors. Unless the reviewers did this, the priors affected by the EMF-specific information would be falsely inflated and there would be a falsely smaller difference between the priors and the independently elicited posteriors based on EMF-specific information.

After taking a workshop on probability elicitation, the reviewers developed an initial prior and then challenged each other as to the rationales for their respective priors. The main lines of argument are reproduced below. The three reviewers first asked themselves:

> How probable is it that the EMF mixture (comparing the 95^{th} percentile or above to the 1^{st} percentile or below) of residential exposure in the United States is capable of altering the risk of one or more types of cancer or other disease with a relative risk between X and Y? These relative risks should be detectable by epidemiology.

Ideally, one would like to answer this question for a series of relative risks, ranging from those suggesting a protective effect (Relative Risk < 0.95) to those with virtually no effect, (RR = 0.95–1.05), and including levels of increasing risk (RR = 1.06 –1.19), (RR = 1.2–1.95), (RR =1.95–2.95), (RR = 2.95–4.95), and (RR ≥ 5). That is, one would like to draw a distribution of prior probabilities for all possible relative risks conveyed by the 95^{th} percentile or above exposure within a typical residential setting relative to the lowest risk exposure. A histogram of these probabilities would have an area of 1.0.

By necessity, the reviewers have not specified exactly what should be contrasted, that is, what aspect of the mixture of the EMF exposure (e.g. what frequency), what summary exposure metric (e.g., time-weighted average (TWA)), or what levels of that metric (e.g., 2 milliGauss (mG) vs. 0 mG). The reviewers have been vague in the same sense than an epidemiologist might be vague about aspects of red wine (alcohol content, grape type, aging, sediment) dosages and dosing patterns when she asks:

> "How probable is it *a priori* that red wine consumed in the usual amounts might alter the risk of cardiovascular disease with relative risks ranging from X to Y?"

Thus, the reviewers conceptualize this general prior probability distribution as if it related to exposures to the whole EMF mixture.

By querying one's prior beliefs, one can begin to anchor the graph of probabilities in various ways:

> How much of the distribution is concentrated around a RR between 0.99 and 1.01, because a) there is really no effect at all, or b) any effect, whether beneficial or harmful, would be virtually negligible?

> Is the graph symmetrical, that is, is it equally likely that EMFs increase or decrease the rate of disease?

> Where does the distribution "start" and "stop"? That is, given what we know about temporal patterns of disease after the introduction of electricity, are

we comfortable assigning non-negligible probabilities to very protective or very deleterious relative risks? Could the usual range of EMF exposure have increased or cut the disease rate by a factor of 100? 50? 25? 5?

Assuming that the epidemiologically detectable RR is about 1.2, is the probability of an EMF effect above this limit vanishingly small? If so, that anchors the graph even further. If not, what does the curve look like above RR=1.2?

2.2 WHAT DO TRENDS IN NATIONAL STATISTICS DO TO BOUND THE UPPER LIMIT OF AN EMF EFFECT?

With a few notable exceptions (see discussion below of childhood leukemia), a large percentage increase in non-infectious diseases during the century that electricity was gradually introduced across the United States and in the world has not been documented. This fact can serve to establish an upper bound for the possible risk from EMFs for the many diseases whose incidence did not increase.

Environmental agents tend to have a skewed distribution of exposure, with most people at the lower levels of exposure and a thin "tail" of people at the highest exposures. This means that comparing people above the 95th percentile of exposure to people below that level is a comparison with a group that is mostly comprised of people with very low exposures.

Environmental epidemiology rarely has the ability to detect a dose-response pattern more refined than a kind of step function with some risk at the very highest levels of exposure, such as the 95th percentile, when compared to all other levels of exposure or to the lowest percentiles of exposure. If EMFs produce detectable effects, it would not be surprising if that pattern were to emerge.

How high would the RR conveyed by the 95th percentile have to be before it would substantially affect the overall rate of disease? One can answer this by calculating something called the Population Attributable Risk Percent (PAR%), the percentage fall in the overall rate of a disease of interest if EMF "exposure" contributing to that disease rate were removed.

It can be expressed as:

$$PAR\% = 100 * \{ (PrU + PrE * RR) - 1 \} / (PrU + PrE * RR)$$

Where PrU = probability of being unexposed

Pr E = probability of being exposed

RR = relative risk conveyed by exposure.

Figure 2.1.1 shows PAR% as a function of the relative risk conveyed by the 95th percentile.

If the 95th percentile conveys a barely detectable relative risk of 1.2 relative to persons exposed below that level, the PAR% is a few percentage points. If it conveys a relative risk of 2, the PAR% is about 5%. Once it conveys a 5-fold relative risk, it accounts for 20% of the overall rate—a detectable effect. It must convey a RR of 21 for EMFs to account for 50% of the current overall rate. This would be the point at which removing the 95th percentile exposure would cut the overall disease rate in half. So, the reviewers' *a priori* confidence in relative risks above 5 or below 1/5 is quite low; but it could be higher for values between these two values because such effects would not be easily noticed.

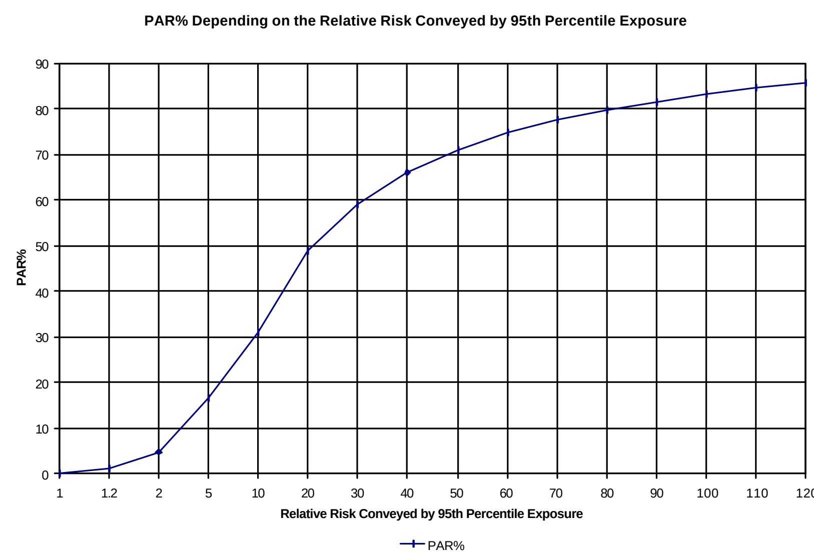

Figure 2.1.1

What if EMFs were very unusual for environmental agents and showed a step function of risk at quite low exposures, say the 25th percentile of exposure? Figure 2.1.2 shows the PAR% as a function of the RR conveyed by the 25th percentile of exposure. A RR of 2 now produces an obvious 40% impact on any disease that is routinely tracked, and a RR of 5 now produces an 80% impact.

So, for diseases that are tracked by vital statistics or special registries and have not changed much, we can say that it is unlikely that EMFs have even modest effects in the lower ranges of exposure. But, if they behave like many other environmental agents, and only display effects at the upper percentiles of exposure, they could convey a RR between 1.2 and 5 without producing obvious impact on overall rates as the use of electricity spread.

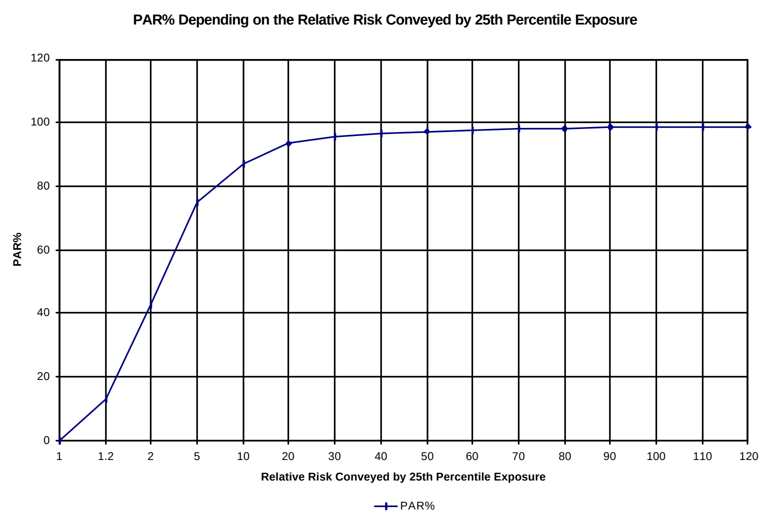

Figure 2.1.2

2.3 THE SPECIAL CASE OF CHILDHOOD LEUKEMIA

Milham (Milham & Ossiander, 2001) drew attention to something that Court Brown and Doll (Brown & Doll, 1961) had pointed out more than forty years ago, that an increased risk of leukemia mortality for 2- to 4-year-old children first appeared in the 1920s and increased in intensity in the 1940s. Thus some factor(s)—perhaps electricity, perhaps accuracy in diagnosis—in those modernized locations caused the registration of toddler leukemia deaths to increase threefold. The evidence from Court Brown, Doll, and others that childhood leukemia mortality registration had indeed increased during the early 20th century increased the prior probability of a moderately large EMF effect, at least for childhood leukemia. This meant that the prior probability of a moderate effect for childhood leukemia was larger than for other diseases.

2.4 ARRIVING AT A PRIOR DEGREE OF CERTAINTY

As explained above, the prior represents the credibility of the hypothesis before hypothesis-testing research was undertaken. It is based only on past experience in analogous situations and on general scientific knowledge. Therefore, the reviewers exclude from this original consideration any epidemiology, experimentation, or exposure research that has been specifically targeted at the power-system EMF hypothesis. The reviewers include in their consideration theoretical estimates of a threshold for environmental EMF impact on biological systems as calculated using basic biological and physical theory because, in principle, these theoretical arguments could have occurred at any time in the recent past, devoid as they are of any empirical input. The reviewers summarize, below, arguments that would tend to increase or decrease one's initial degree of confidence that exposures could influence risk.

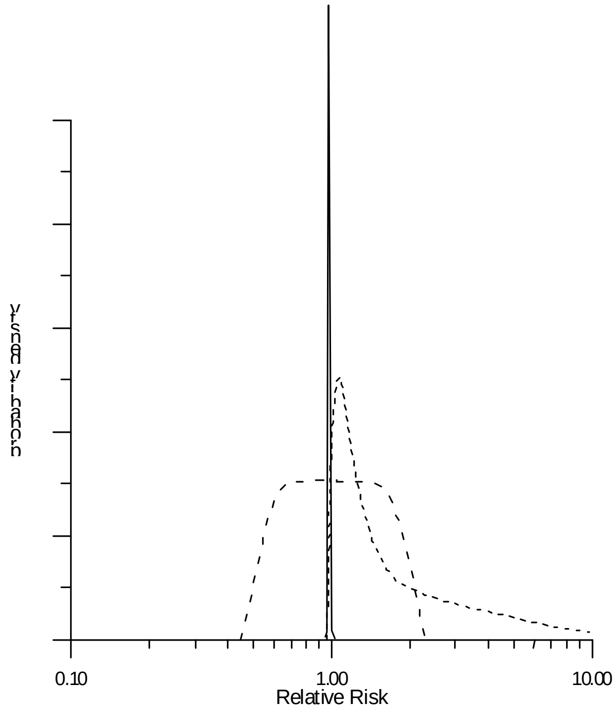

Figure 2.1.3

The DHS reviewers developed arguments in favor and against three possibilities (Figure 2.1.3):

1) A probability distribution of the prior that is symmetrical and has a large variance, suggesting that beneficial and harmful effects are equally likely (indicated by long dashes).

2) A probability distribution of the prior that is tightly clustered around a relative risk of 1, essentially no effect (indicated by a solid line).

3) A probability distribution of the prior strongly skewed toward relative risks of a harmful nature (indicated by short dashes).

In discussing the distribution of the *a priori* probability of risk, the reviewers refer to 50–60 Hz EMFs as an "extraneous" environmental agent. They define an extraneous agent as one that either is totally extraneous to the evolutionary environment or is present in abnormal concentrations and forms (e.g., lead, refined from the mineral galena, its natural form, and introduced in industrial products).

An extraneous agent is not to be confused with an impurity. Drinking water is full of components other than H_2O, but most of these were present over the billions of years life has evolved on Earth. The question, "What percentage of impurities found in today's water supplies should people be concerned about?" may well have a different answer from, "What percentage of impurities in today's water that were not there during evolutionary times should people be concerned about?"

2.4.1 ARGUMENTS FOR A PROBABILITY DISTRIBUTION OF THE PRIOR THAT IS SYMMETRICAL AND HAS A LARGE VARIANCE

<u>Argument</u>

In the absence of evidence, one should keep an open mind and allow that, although extreme protective effects or extreme risks are very unlikely, because the consequences would have become apparent without targeted research, moderate protective effects or moderate risks are both possible and equally likely.

<u>Rebuttal</u>

Agents that are beneficial for the whole, or at least the vast majority of the population (e.g., fresh fruit) are so because the human body has evolved to make use of what is available in the environment. Many environmentally extraneous agents are also beneficial (e.g., mineral supplements) but only to those individuals who need their specific properties. Although we add fluoride to drinking water and iodine to table salt, we do so in concentrations similar to those found in nature in some (but not all) water sources and in marine salt. The reviewers cannot think of a single factor that is totally extraneous, environmentally, and that people would consider adding to the water supply or disperse in the environment trusting that it would benefit at least some section of the population without harming other sections.

2.4.2 ARGUMENTS FOR A DISTRIBUTION OF THE INITIAL DEGREE OF CONFIDENCE TIGHTLY CLUSTERED AROUND A RELATIVE RISK OF 1 (NO EFFECT)

<u>Argument</u>

Environmental EMF levels induce fields and currents that are orders of magnitude lower than endogenous fields and currents in living organisms. It is true that some animals can perceive very weak electric and/or magnetic fields, but these require highly specialized organs, which these animals evolved to take advantage of variations in the geomagnetic field. Precisely because EMFs in the extremely low frequency range (50–60 Hz) are man-made, there was no reason or opportunity for the body to develop a detector of electric or magnetic fields at these frequencies.

Such organs, in species where they are found, are relatively large and complex. There is no reason to believe that such an organ in humans could be so simple and small as to be so far undetected.

Therefore, theory indicates that EMFs can have no biological effect and therefore no pathological effect. Notice that the ignorance about a possible physical induction mechanism for residential-intensity EMFs is qualitatively different from the ignorance we have about the exact physicochemical mechanisms for chemical carcinogens or the exact physical interaction with an asbestos fiber. In the EMF case, what little IS known suggests that no effect should be happening and we cannot build a physico-biological model that predicts a biological effect at ambient levels. With other agents, a variety of plausible mechanisms are known, but it is not known if one of them is at work.

Even assuming that EMFs can be perceived above noise and that a coupling mechanism exists, the amount of energy transferred to the body would be so small that any effect must be trivial and easily tolerated. The effects of residential exposures to other agents have rarely been detectable by epidemiological methods.

For other physical agents that are known to cause harm, the mechanism by which physical energy initiates a cascade of chemical or biological events is understood. One physical mechanism by which electromagnetic radiation could cause cancer is the breaking of molecular bonds if the photon energy is sufficiently high. Other adverse effects (e.g., radio frequency EMF (RF) burns) are due to the heating of tissue and the induction of relatively large currents. None of these mechanisms occurs with exposure to environmental 50–60 Hz EMFs at residential or even blue collar exposure levels. No other mechanism has been identified which could lead from biological change (even if biological change were possible) to physiological or pathological results that would cause us to believe there would be an effect.

For these reasons the prior for any effect except, at most, very small ones should be virtually zero.

<u>Rebuttal</u>

Modern science is based on observation and experimentation. Theory cannot "prove" anything. It can only explain or predict observation. The physio-biological models that predict no effect is possible are sophisticated on the physics side but may be incomplete on the biology side.

Man-made 50–60 Hz fields are extremely regular: macroscopic changes in intensity and direction are negligible on the time-scale of the sinusoidal oscillations. Because of their time coherence (e.g., the regularity of the frequency) they might be distinguished from random noise, using a comparable time reference. This would not necessarily require a resonance but simply a time marker against which the regularity of these fields could be verified.

Because of their space coherence (e.g., the fact that the crests and troughs of these waves reach all parts of the body at the same time) billions of cells are stimulated simultaneously. These weak but numerous stimuli may add together to produce a detectable signal.

Although the human body had no evolutionary incentive to develop a detector to use 50–60 Hz EMFs, it is possible that these man-made frequencies are perceived as a perturbation of the status quo. By analogy, a radio set is not designed to detect electromagnetic interference from an appliance but does so, with a resulting adverse effect to the radio's proper function.

The way the human body may detect these oscillating, extremely regular signals bears no relationship to the way magnetic organs in some animals detect static fields. The shape and size of these organs is not necessarily relevant to predict the shape and size of a 50–60 Hz detector.

The only well-understood effects of electromagnetic radiation are those deriving from the breaking of atomic and molecular bonds, the heating of tissue, and the induction of electrical currents. Nevertheless, there was vast, if controversial, scientific literature even before 1979 (the time when the Wertheimer and Leeper study was published) that argued there were observed health effects from radio frequency EMFs, for which there was no mechanistic explanation. [For a critical summary, see Steneck, "The Microwave Debate."] EMFs are not unique in this respect. Many carcinogens and reproductive toxicants act by unknown mechanisms. For example, the physical-induction mechanisms responsible for the effects of ultra-violet (UV) light are not fully understood either.

It is not known if energy is the appropriate measure of dose. Radio signals reaching a radio antenna a have very low energy level but are adequate to make the radio work. A weak stimulus may be all that is required to trigger a stronger, secondary effect.

Discussion and Conclusion

Since the inception of modern science, the role of theory has been not to prevail over observation, but rather to explain and predict it. Both in ancient and modern times, there are numerous examples of theories being proven wrong and models being proven inadequate. One cannot put too much trust in the theory-based belief that EMFs cannot be distinguished from noise and, therefore, cannot produce biological or pathological effects.

2.4.3 Arguments In Favor of a Distribution of Prior Probabilities Strongly Skewed Toward Relative Risks of a Harmful Nature

One should be suspicious of extraneous environmental factors. Living organisms are complex entities that, over billions of years, developed opportunistically to maximize the benefits and minimize the damages of the agents making up the environment in which they exist. They have had no time to evolve specific defense mechanisms (e.g., specific detoxifying enzymes) against extraneous agents. Moreover, in the case of something so totally artificial as 50–60 Hz EMFs, they do not even have general repair mechanisms (such as detoxifying enzymes developed for a naturally occurring different, but chemically similar, agent) or simple aversion reflexes, such as blinking or coughing.

Electric currents play a vital role in normal physiological functions. EMFs induce electric currents and therefore have the potential to seriously disrupt a vast range of biological functions.

Even if low on a physical scale of measure, environmental levels of EMFs at 50–60 Hz are potentially a massive biological dose, representing a many-order-of-magnitude increase over the virtually insignificant levels existing in the natural environment.

In the absence of specific evidence as to dose, it is reasonable to assume that the probability of an adverse effect is higher for a small risk than for a large one, and that it becomes vanishingly small for values of the risk so large as to make it inconsistent with the information gleaned by environmental health monitoring (RR 5, according to standard calculations). Therefore, a distribution of prior probabilities positively skewed should be accepted, with a mode close to, but greater than, 1.

Rebuttal

It seems unreasonable that all extraneous agents would be harmful, particularly at low ambient levels. Using the criterion that at least 1 of 4 standard bioassays was positive (male and female rats and mice), Fung et al. (Fung et al., 1993) summarized the carcinogenicity of 379 chemicals as 68%, 37 “natural agents” as 40%, and 126 agents chosen primarily on volume of use as 21%. So “natural” agents were not less carcinogenic than agents chosen at random.

One ought to think quantitatively about detection limits and dose. Just because aspirin is capable of treating headaches does not mean that one aspirin tablet added to the city's reservoir will cure all the headaches in town. That 21% of chemical agents chosen primarily on the volume of use can produce cancer in laboratory animals at the highest tolerated dose does not mean that very low doses of the same agent in the environment will produce ***epidemiologically detectable*** cancer. Perhaps none of these chemicals has a threshold of effect, but each is increasing the risk to some small degree, even though not enough for an epidemiologist to detect. A very small proportion of the 21% would produce effects from low environmental exposures that could be detected by epidemiologists, and this is equally true for “natural” and “extraneous” agents.

2.5 Conclusion of the Core Evaluators

Reviewer 1

On the basis of the arguments for a high or a low prior for biological effects, Reviewer 1 believes that the probability that environmental EMFs are beneficial is very small because of the extraordinary coincidence that would be required for a complex organism to benefit from something that was totally absent during its evolutionary development. The probability that extraneous electrical signals leave an organism that depends on electrical signals for its proper functioning totally unperturbed also is very small. The question is one of dose and size of effect. If the dose and the resulting response are small and easily tolerated (not repaired, because Reviewer 1 has no basis to believe that repair mechanisms against an unknown and totally alien agent may have evolved by accident), then pathological results could be seen only in a very few subjects who, either by chance or extraordinary vulnerability, are not able to tolerate these small effects. (This is analogous to saying that exposure to a common cold virus carries a very small risk of death). Reviewer 1 believes that this scenario has a very high probability. However, this probability is not close to unity because the dose may be considered in relative terms. In this case, the reviewers are justified in believing that an increase from virtually zero to several mG represents a massive increase in dose that is not easily tolerated. In broad terms, Reviewer 1 believes that the *a priori* probability that EMF has little or no effect is large (about 85%) and that the probability of a beneficial effect is considerably smaller (say, about 3%) than that of a moderate (RR < 5) risk (about 12%).

Reviewer 2

Reviewer 2 was not much swayed by arguments linking physical principles to simplified biological models which predicted that no biological effect and no pathological effect would be possible from residential and occupational exposures to the EMF mixture. The EMF mixture was, thus, only slightly less likely to cause harm than any other randomly chosen agent about which one initially has little specific information. The initial lack of mechanistic information or relevant animal pathology evidence was similar to that of all members of the class of agents about which little is known. And effects of regulatory concern could have been occurring without being noticed if, like other environmental agents, the risk were barely detectable by epidemiology and confined to the upper percentiles of exposure. It seemed reasonable that extraneous agents were somewhat more probable to produce harm than agents prevalent in the environment during the course of evolution, but Reviewer 2 thought that even such agents as these were more likely to produce no detectable effect at all. The fact that electrical and magnetic phenomena are involved in normal physiology also argued somewhat for the possibility that the EMF mixture might have biological or pathological effects. But even if Fung et al. (Fung et al., 1993) are correct, that agents chosen at random have a 20% chance of producing a noticeable pathological effect at high dose and some effect at ambient doses, perhaps a quarter of those (say 5%, range 1%–20%) produce effects at low doses that epidemiologists can see with relative risks (say, between 1.2 and 5.0) or their reciprocal on the protective side. More of that 5% (3 or 4%) would be on the harmful (RR > 1.2) rather than the beneficial (RR < 0.8) side, on the basis of the "extraneous agent" arguments.

This is tantamount to saying that the probability of no epidemiologically detectable effect at any dose would range from 80% to 99%, with a best estimate at 95%.

The prior probability of relative risks above 5.0 or below 0.2 seemed extremely small.

Reviewer 3

Reviewer 3 believed that environmental (residential and occupational) EMFs are exogenous agents, for all practical purposes, nonessential for normal human function. This is because they are man made and added by human activity resulting from an increase in electricity use correlated with industrialization. Hence, the probability of a prior protective nature of EMFs is very small. Reviewer 3 believed that environmental EMFs convey some health risk, since they are composed of a mixture of a variety of components, where any one or several of the components may interact with a number of biological processes and result in an adverse health effect. The probability of any effect greater than a relative risk of 1.0 is 17% (median value) with a range of 5% to 37%, with a very small probability of relative risks above 5. These distributions are based on the fact that 1) most diseases are multifactorial in nature, 2) adverse health effects associated with environmental agents may be subtle and have long induction periods, and 3) information about the relevant biological EMF agent(s) and their associated dose are not known.